

# **EXHIBIT 19**

**IN THE UNITED STATES DISTRICT COURT  
FOR THE DISTRICT OF MARYLAND**

MARYLAND SHALL ISSUE, INC., *et al.*, \*

*Plaintiffs,* \*

v. \* Civil Case No. 16-cv-3311-ELH

LAWRENCE HOGAN, *et al.*, \*

*Defendants.* \*

\* \* \* \* \*

**THIRD SUPPLEMENTAL DECLARATION OF DANIEL W. WEBSTER**

I, Daniel W. Webster, under penalty of perjury, declare and state:

1. I am Bloomberg Professor of American Health in Violence Prevention in the Department of Health Policy and Management at the Johns Hopkins Bloomberg School of Public Health with a joint appointment in the School of Education. I am also Director of the Johns Hopkins Center for Gun Policy and Research and previously served as Co-Director of the Johns Hopkins Center for the Prevention of Youth Violence. I am more than 18 years of age and am competent to testify, upon personal knowledge, to the matters stated below.

2. This declaration is submitted in response to the supplemental declaration of plaintiffs' expert Gary Kleck (ECF 135-29) filed in support of plaintiffs' cross-motion for summary judgment, which attempts to critique my expert opinions in this case and some of the research studies upon which my opinions are based.

3. Kleck claims that the three studies that I have published with colleagues demonstrating that the repeal of Missouri’s handgun permit-to-purchase (PTP) law (also known as handgun purchaser licensing) in 2007 was associated with a large and statistically significant increase in firearm homicides is a matter of “data dredging.” [We also found similarly large and statistically significant increases in firearm suicide coincident with this same policy change (McCourt et al., 2020)<sup>1</sup>, a 2-fold increase in the share of crime guns that moved unusually quickly from retail sale to crime involvement in Missouri (Webster et al., 2013)<sup>2</sup> and a 37 percent increase in the number of guns originally sold in Missouri that were later recovered in neighboring states of Illinois and Iowa with PTP laws (Webster et al. 2014)<sup>3</sup>, and a more than two-fold increase in the rate of law enforcement officers nonfatally wounded in the line of duty by criminal suspects using handguns (Crifasi, Pollack & Webster, 2016)<sup>4</sup> – all conspicuously co-incident with Missouri’s repeal of its

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<sup>1</sup> McCourt, Alexander D., Cassandra K. Crifasi, Elizabeth A. Stuart, Jon S. Vernick, Rose M.C. Kagawa, Garen J. Wintemute, and Daniel W. Webster. (2020) Effects of Purchaser Licensing and Point-of-Sale Background Check Laws on Firearm Homicide and Suicide in Four States. *American Journal of Public Health* 110:1546-1552. doi: 10.2105/AJPH.2020.305822. Epub 2020 Aug 20.

<sup>2</sup> Webster, Daniel W., Jon S. Vernick, Emma E. McGinty, and Ted Alcorn. (2013) “Preventing the Diversion of Guns to Criminals through Effective Firearm Sales Laws,” pp. 109-122 in Webster, Daniel W. & Jon S. Vernick, Eds. *Reducing Gun Violence in America: Informing Policy with Evidence and Analysis*. Baltimore, MD: Johns Hopkins University Press.

<sup>3</sup> Webster, Daniel W., Cassandra K. Crifasi, & Jon S. Vernick. (2014) Effects of the repeal of Missouri’s handgun purchaser licensing law on homicides. *J Urban Health* 91:293-302. Erratum: *J Urban Health* 91:598-601.

<sup>4</sup> Crifasi, Cassandra K., Keshia Pollack, & Daniel W. Webster. (2016) The influence of state-level policy changes on the risk environment for law enforcement officers. *Injury Prevention* 22:274-8. doi: 10.1136/injuryprev-2015-041825. PMID: 26718550.

PTP law.] Kleck implies that my colleagues and I poured over decades of data and many changes in gun laws and found one by chance that demonstrated an association, for a particular period of time, between a change in a PTP law and firearm homicide rates. But the research I cite in my declarations reveal many points of evidence consistently linking multiple changes in PTP laws over a range of time periods with changes in firearm homicides and other key outcomes that are relevant to public safety and that are consistent with prior theory and evidence about the role of handgun purchaser licensing laws. Kleck's argument that the evidence I put forward is based on a single outlier data point (Missouri's firearm homicide rate in 2008) is simply not true. My conclusions are supported by peer-reviewed articles published in top scientific journals that use multiple sources of data and multiple analytic techniques commonly used to estimate the effects of change in public policies on public safety outcomes, and multiple changes in PTP laws with remarkably consistent findings.

4. Kleck criticizes the first scientific article I published that demonstrated a strong and statistically significant association between the repeal of Missouri's PTP law (Webster et al., 2014) and an increase in firearm homicide rates because I did not look at more changes in PTP laws over a longer period of time. He fails to mention that my co-authors and I provide a strong rationale for using data beginning in 1999. The 15 years prior to 1999 included the most dramatic changes in firearm homicide rates in decades. The timing and magnitude of those changes were (1) far from uniform across states and (2) appear to have been driven by the emergence and turbulence of crack cocaine markets that are not easily measured in a standardized way that would readily allow for reliable direct

statistical controls. Importantly, the states most impacted by crack cocaine and its lethal firearm violence were far more urban and with more densely populated cities with highly-concentrated poverty among African Americans, and tended to have stricter gun laws than more rural states. Thus, the omitted and largely unmeasurable (in a standardized way) variable of volatile crack cocaine markets can easily bias estimates of the effects of gun laws enacted during this period (Donohue, Aneja & Weber, 2019).<sup>5</sup>

5. Kleck suggests that any association that my research found between the repeal of Missouri's repeal of its PTP law and increased firearm homicide rates may have been due to a temporary increase in gang violence in a single city, e.g., St. Louis. The data tell a very different story. Comparing changes from the relatively stable period of 1999-2006, before Missouri repealed its PTP law on August 28, 2007, and the period after the law had been repealed (2008-2019), the average annual firearm homicide rate in St. Louis (its own county) increased by an astronomical rate from 22.3 to 33.9 per 100,000 population, a per capita increase of 11.6 firearm homicides per 100,000. That increase is nearly three times higher than that of the large central metro county with the next highest per capita increase over that period, Jackson County, Missouri (which includes Kansas City) where annual firearm homicide rates increased from 11.6 to 15.6 per 100,000 population. Among the 64 large central metro counties in the United States, the two jurisdictions that experienced the largest increase in firearm homicide rates during the 12-

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<sup>5</sup> Donohue, John J., Abhay Aneja, & Kyle D. Weber. (2019) Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis. *Journal of Empirical Legal Studies* Volume 16, Issue 2, 198–247.

year period after Missouri repealed its PTP law were both located in Missouri, St. Louis City (which is its own county) and Jackson County where Kansas City is located. Looking at all large central metro counties in the United States over these time periods, the average annual firearm homicide rate *declined* from 6.92 during 1999-2006 to 5.89 during 2008-2019, a decrease of 1.03 firearm homicides per 100,000 population (15 percent). Thus, the change in population risk of death from firearm homicide in Missouri's urban counties over the 12 years full years after the state repealed its PTP law was an extreme outlier compared to national trends.

6. The increases in firearm homicide in Missouri following the repeal of its PTP law were not only an urban phenomenon. Other less urban counties saw their annual average firearm homicide rate increase from 2.37 to 3.73 between the period in which the PTP law was in place (1999-2006) and the 12-year period after the repeal (2008-2019), an increase of 36.4 percent. The increased risk for firearm homicide among Missouri residents was more than three times as large as what was experienced nationally for non-large-central-metro counties over those time periods (2.75 during 1999-2006 and 3.08 during 2008-2019). In summary, the large increase in firearm homicide that Missouri residents experienced after it repealed its PTP law relative to changes throughout the rest of the U.S. were not limited to a single jurisdiction or to a single year (2008)<sup>6</sup>; the surge in firearm homicides occurred throughout the state over the first 12 full years following the repeal. Furthermore, the rise in lethal violence did not abate in Missouri in 2020. In St. Louis,

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<sup>6</sup> Rice, Glen and Kansas City Star.  
<https://www.kansascity.com/news/local/crime/article246077040.html>

homicides increased by 35 percent from 2019 to 2020, causing the homicide rate to be at a 50-year high (Hefferman, 2021).<sup>7</sup> Kansas City had its highest homicide rate ever in 2020 as well (Willis 2021)<sup>8</sup>. Kleck's claim of a happenstance, 1-year blip in 2008 attributable to some local phenomenon could not be more discordant with the available data and contradicts the research he cites.

7. In addition to an increase in firearm homicide rates, the repeal of Missouri's PTP law also was coincident with the two-fold increase in the share of crime guns recovered in Missouri (Webster et al., 2013). Guns recovered from a criminal suspect or a crime scene less than 12 months after retail sale is widely accepted as an indicator of criminal diversion by the top researchers studying gun violence (Braga et al., 2012).<sup>9</sup> Missouri experienced a 37% increase in guns that were recovered from criminal suspects and crime scenes than neighboring states with PTP/handgun purchaser licensing laws

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<sup>7</sup> Hefferman, Erin. (2021) St. Louis homicide rate in 2020 highest in 50 years with 262 killings. St. Louis Today. January 1. [https://www.stltoday.com/news/local/crime-and-courts/st-louis-homicide-rate-in-2020-highest-in-50-years-with-262-killings/article\\_b3c323a7-bc38-55bc-812b-08990b0eb289.html](https://www.stltoday.com/news/local/crime-and-courts/st-louis-homicide-rate-in-2020-highest-in-50-years-with-262-killings/article_b3c323a7-bc38-55bc-812b-08990b0eb289.html)

<sup>8</sup> Willis, Jasmyn (2021) St. Louis 2020 homicide rate is the highest in 50 years and KC has deadliest year ever. KRCG-TV <https://krcgtv.com/news/local/st-louis-2020-homicide-rate-is-highest-in-50-years-kc-suffers-deadliest-year-ever#:~:text=But%20because%20the%20city's%20population,highest%20on%20record%20since%201970.>

<sup>9</sup> Braga, Anthony A., Garen J. Wintemute, Glenn L. Pierce, Philip J. Cook, and Greg Ridgeway (2012) Interpreting the empirical evidence on illegal gun market dynamics. *Journal of Urban Health* 89(5):779-93. DOI 10.1007/s11524-012-9681-y. Kleck cites an article he wrote with Shun-yung Wang that was published in the UCLA Law Review in 2009 as "evidence" that the indicators that I and other leading researchers use to measure the diversion of gun from lawful to criminal use are invalid because they do not correlate well with their preferred indicator – obliterated serial numbers. Of course, a law review article is a far cry from scientific peer review. Professors Braga, Wintemute, Pierce, Cook and Ridgeway represent some of the top researchers studying gun violence and illegal gun markets. The article referenced here is a scathing rebuttal of Kleck & Wang's claims published in an actual scientific peer-reviewed journal.

(Illinois and Iowa). This represented a clear shift in the share of guns recovered by law enforcement in Missouri that originated with a retail sale within the state (Webster et al. 2013). The timing of these changes in the diversion and criminal misuse of firearms in Missouri also coincided with sharp increases in firearm homicide rates in the state relative to comparison states and controlling for other factors associated with suicide rates (Crifasi et al., 2015;<sup>10</sup> McCourt et al., 2020). Each of these data points are consistent with the theory that requiring a law enforcement-issued permit to purchase a handgun protects against illegal straw purchases, firearm trafficking, and impulsive high-risk handgun acquisitions with the potential to lead to lethal consequences. Kleck has not offered any explanation for the huge and statistically anomalous increases in multiple forms of firearm mortality that have occurred in Missouri over a twelve-year period nor did he explain the stark changes in the diversion of guns for criminal use shortly after retail sale by Missouri firearms dealers that are also co-incident with the repeal of Missouri's PTP law.

8. Kleck offers a variety of warnings that the many estimates of the changes post-PTP-repeal that we find to be statistically significant may be attributable to unmeasured omitted variables. The truth is that studies of this type cannot directly measure all important factors that could potentially influence changes in firearm homicide rates because we do not have standard ways to measure conditions such as gang feuds or important changes in local drug markets. That is why we must identify the best available

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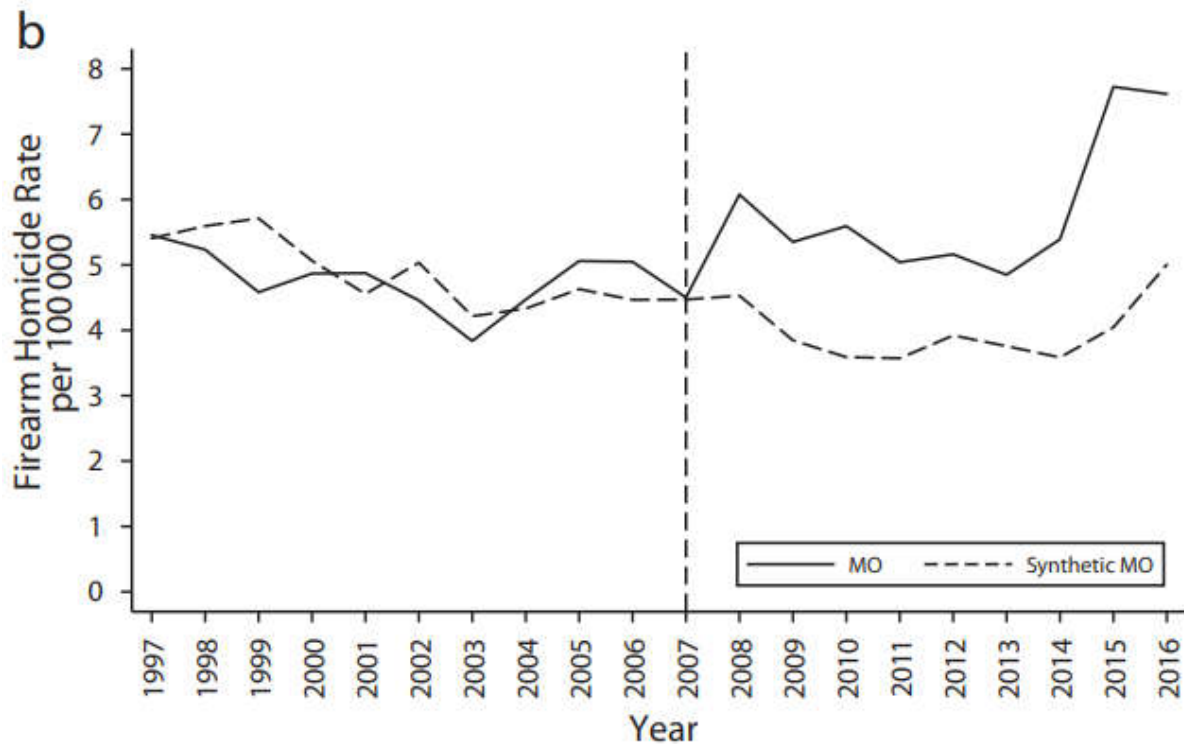
<sup>10</sup> Crifasi, Cassandra K., John Speed Meyers, Jon S. Vernick, and Daniel W. Webster. (2015) Effects of changes in permit-to-purchase handgun laws in Connecticut and Missouri on suicide rates. *Preventive Med.* Jul 23, 2015. pii: S00917435(15)00229-7. doi: 10.1016/j.ypmed.2015.07.013.



comparison states that are most likely to share similar unmeasured conditions that influence homicide rates and closely examine the data for consistency in the associations and base our estimates on *differential* changes in homicide rates between states that change PTP laws and similar states with similar baseline trends. Those similarities in baseline levels and trends between states that adopt new gun laws of interest and those that do not adopt those laws of interest, especially when preceded by a period when homicide rates are not particularly volatile, provide assurance that estimates of the impact of gun laws based on differential changes in homicide rates coincident with changes in laws are attributable to changes in gun laws. The 1999-2007 period prior to the repeal of Missouri's PTP law was a very stable period for annual firearm homicide rates, ideal for isolating the effects of a very meaningful change in state firearm policy. Why did we only examine this particular change in PTP law at that time of the first study isolating the change in Missouri's PTP law in Webster et al., 2014? As stated in the article, it was the only change in a PTP law during that time period. Another advantage to studying the repeal of a law as opposed to its initial implementation is that most statistical methods are best designed to contrast abrupt changes – legal requirements and procedures immediate change – as opposed to a less certain process of ramping up implementation of a new law.

9. One of the articles that assesses the changes in firearm homicides in Missouri relative to changes in other states in the region uses sophisticated methods to identify the most appropriate comparisons for the state with a significant policy change. This was published in one of the top journals for research methods in public health and safety, *Epidemiology*, (Hasegawa, Webster & Smart, 2019) and was recognized by the journal as

the second most outstanding research methods contribution of all of the articles published in 2019. The study showed that when compared with states within the region with baseline rates of firearm homicide comparably as high as Missouri's, Missouri's PTP repeal was associated with a 27 percent increase in firearm homicide rates through 2016. Our most recent research study uses another sophisticated analytic method designed to develop the most accurate counterfactual forecast for the purpose of deriving estimates of the effects of a policy change on relevant outcomes – synthetic control methods – and demonstrates that the approach produces highly-accurate prediction of Missouri's firearm homicide rates prior to the PTP repeal by using a weighted combination of comparison states and covariates that maximize prediction accuracy (McCourt et al., 2020). The synthetic control models revealed close concordance between Missouri's actual firearm homicide rates and its synthetic control until the PTP repeal went into effect. During 2008-2016, there was an immediate and consistent gap between Missouri's firearm homicide rate following the repeal and the forecasted counterfactual (see figure below). Based on this immediate and consistent difference between actual firearm homicide rates and those predicted by the synthetic control, we estimated a 47 percent increase in firearm homicide rates in Missouri associated with the policy change. What would explain such a large change in firearm homicide rates that was exactly coincident with PTP repeal and last for more than a decade?

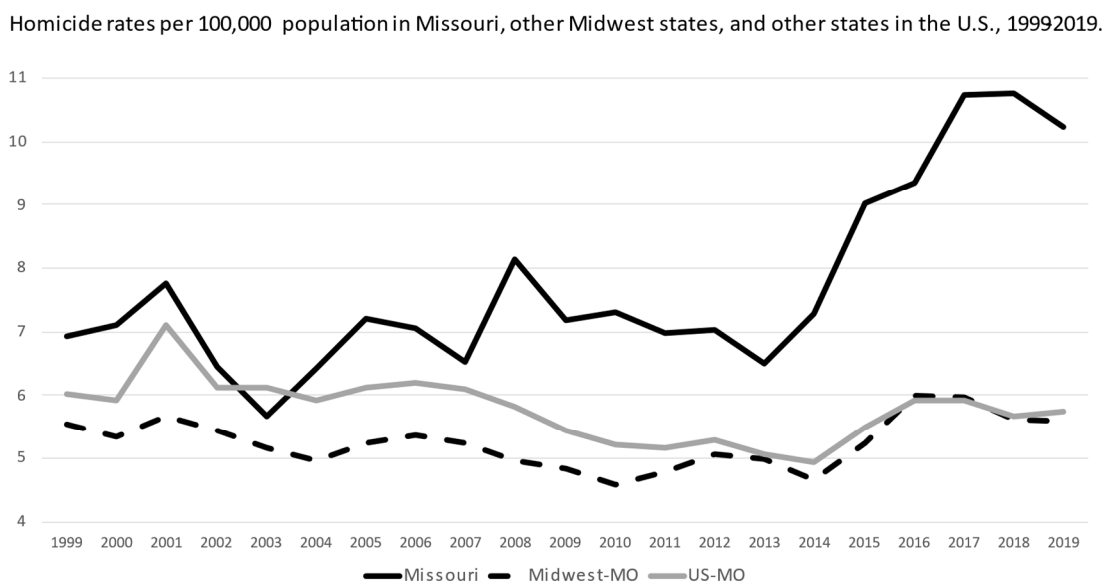


10. Kleck incorrectly argues that, because we found no protective effect of comprehensive background check (CBC) laws in Maryland (1996) and Pennsylvania (1995) that did not include purchaser permit/licensing requirements, that the repeal of Missouri's PTP law could not have impacted homicides due to criminals' acquisition of handguns from private parties. Our contention is that the addition of purchaser permit requirements, in particular requiring a prospective purchaser to apply directly at a law enforcement agency (as was the case in Missouri) and/or requiring purchasers to be fingerprinted and complete safety training (as is the case in Connecticut), is likely to be a significant deterrent to one of the most common ways in which prohibited purchasers obtain firearms – through straw purchases or from private unlicensed sellers who purchase firearms from licensed gun dealers and then sell them in the underground gun market to

criminals (Braga et al., 2012). In the same paragraph (34 on page 15), Kleck appears to suggest that the primary way to determine whether the repeal of Missouri's law impacted firearm homicide rates is to examine whether background checks led to increased denials due to criminal prohibitions on applicants. This makes no sense. The repeal of PTP eliminated the legal requirement for background checks if the transfer is between private parties and eliminated the legal requirement that handgun purchasers first obtain a permit from the local sheriff. These changes set the stage for virtually eliminating the risk for illegal straw sales that supply prohibited criminals with handguns. Prior to the repeal, a prospective straw purchaser was legally required to engage with the Sheriff's office to be vetted and then had the legal requirement to ensure that anyone who they sold their handgun to also had a valid permit to purchase. Thus, the repeal of these provisions would not lead more prohibited criminals trying to buy handguns from licensed dealers when the repeal made it far easier and less risky to acquire a handgun through a straw purchase and for traffickers to channel guns into the underground market. Indeed, the data from our research studies demonstrating an immediate two-fold increase in the share of crime guns that had been purchased within 12 months of crime involvement doubled following the law's repeal (Webster et al., 2013).

11. Kleck suggests that my research on the impacts of Missouri's repeal of its PTP law does not show that the repeal was associated with a reduction in deaths because the first two studies showed that the repeal was followed by significant increases in firearm homicide rates, but does not examine the repeal's association with overall homicides. Here he is raising the familiar weapon substitution hypothesis, i.e., that persons prone to commit

lethal violence will substitute an equally lethal means of killing someone if access to firearms are restricted. He presents no actual evidence that this weapon substitution theory is supported in research. In the fields of criminology and public health, the heightened lethality of firearms compared to other personal weapons, e.g., objects such as knives, is settled and not controversial. If Kleck's suggestion that homicide rates did not really change in Missouri after the repeal of the state's PTP law, one would expect to see little difference in Missouri's overall homicide trends compared with its regional neighbors or all other states. Yet the data show Missouri's homicide rate increased significantly compared to other states (see figure below with data from CDC's WISQARS Fatal Injury Reports, 1999-2019 <https://webappa.cdc.gov/sasweb/ncipc/mortrate.html>).



The gap between Missouri's homicide rates and its regional neighbors and with the rest of the country grows immediately after Missouri's PTP law is repealed. Comparing the mean

annual homicide rate for Missouri during the first 12 years after its PTP law was repealed (2008-2019) versus its mean annual homicide for the eight years prior to the repeal (1999-2006), Missouri's annual homicide rate increased from 6.82 to 8.37 per 100,000 population, an increase of 22.8 percent. Comparing the same time periods for all other states in the Midwest shows a decline of 2.7 percent (from 5.34 to 5.20) and for all other states shows a 11.6 percent decline (from 6.19 to 5.75). Had Missouri's homicides trends changed similarly to other states in the Midwest during 2008-2019, its annual homicide rate during that period would have averaged 6.64 rather than 8.37. That translates to 1,460 fewer homicides – 122 per year – that would have occurred had Missouri followed the regional trend rather than the one it did. Clearly, in Missouri, homicides committed by means other than firearms did not decline at a rate similar to the increase in firearm homicides after the PTP law was repealed. This was confirmed in the synthetic control statistical models for non-firearm homicide rates in Missouri that we published in McCourt et al. 2020.

12. Kleck correctly states that prior studies that I have been involved in have found no statistically significant protective effect from laws that extend background check requirements to private transfers *but do not also require handgun purchasers to obtain a license or permit to purchase or own a handgun*. That does not mean that background check requirements for firearm sales are unnecessary or irrelevant to firearm violence. Dr. Garen Wintemute, a top researcher with whom I have collaborated, has presented persuasive arguments for why comprehensive background check (CBC) laws alone often yield minimal impacts on firearm fatalities in studies to date which have primarily focused

on law changes in the 1990s or early 2000s. One of the key reasons for this is the inability to both identify prohibited applicants and deter illegal straw purchases without a permit to purchase (PTP) or licensing system in place (Wintemute 2019).<sup>11</sup> PTP or purchaser licensing systems that use law enforcement agencies and fingerprint verification of an applicant's identity more effectively vet applications to purchase firearms than can be accomplished by gun store owners and clerks who process these applications in the absence of licensing systems or biometric identity markers. Background checks conducted with only identification documents but without the applicant's fingerprints can lead to "false negatives" – situations in which individuals are cleared to purchase and possess firearms despite having a disqualifying criminal conviction. One tragic example of this occurred in 2019 when Gary Martin, who had received his Firearm Owner Identification card (FOID) in January 2014 after passing a background check, used a handgun he acquired on March 6, 2014 with that FOID card to murder five co-workers, shoot another co-worker, and shoot five police officers responding to the incident in Aurora, Illinois in 2019. Mr. Martin also died in the incident. Martin should have been denied his application to purchase the handgun he used in this mass killing because he was convicted in 1995 in Mississippi of felony aggravated assault. On March 16, 2014, he applied for a concealed-carry permit which, under Illinois law, requires applicants to be fingerprinted for the background check. His fingerprints flagged him for the 1995 conviction in Mississippi and his application for

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<sup>11</sup> Wintemute, Garen J. (2019) Background Checks For Firearm Purchases: Problems And Recommendations To Improve Effectiveness. *Health Affairs* Vol. 38 (10) doi: 10.1377/hlthaff.2019.00671

a concealed carry permit was denied (Hanna, Karimi, & Almasy, 2019).<sup>12</sup> The FBI and most state law enforcement agencies have invested considerable resources in the development and maintenance of the Integrated Automated Fingerprint Identification System (IAFIS). The FBI uses the IAFIS to link criminal records for individuals because of the public safety risks for failing to link records of individuals who use false names and identification documents or linkage failures due to misspelling names or making mistakes on social security numbers (SEARCH Group, Inc., 1993).<sup>13</sup> As the authors of a General Accounting Office study reported, the “FBI provided us examples of actual cases in which IAFIS responses to law enforcement agencies prevented the premature release of arrested individuals who had used false names and were wanted in other jurisdictions.” (General Accounting Office, 2004)<sup>14</sup>

13. By requiring fingerprint verification of identity and/or in-person law enforcement agency scrutiny of purchaser applications and identification documents, purchasers are less likely to attempt to provide fraudulent or incorrect identifying information upon which the background check is conducted. The more rigorous systems for purchase applications under purchaser licensing versus CBC, in addition to such

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<sup>12</sup> Hanna, Jason, Faith Karimi, and Steve Almasy. “Shooter in Deadly Illinois Rampage was not Supposed to own a gun. CNN, February 18, 2019. <https://www.cnn.com/2019/02/16/us/illinois-aurora-shooting/index.html>

<sup>13</sup> SEARCH, The National Consortium for Justice Information and Statistics. (1993) Use and Management of Criminal History Record Information: A Comprehensive Report. Prepared for the U.S. Bureau of Justice Statistics. <https://www.bjs.gov/content/pub/pdf/CCHUSE.PDF>

<sup>14</sup> General Accounting Office (2004) Information on Timeliness of Criminal Fingerprint Submissions to the FBI. GAO-04-260. <https://www.gao.gov/assets/250/241267.html>



requirements as mandatory safety training, are likely to dissuade individuals from agreeing to purchase firearms on behalf of another person who is legally prohibited or who is planning to use the firearm to commit a crime.

14. The importance of fingerprinting and/or in-person application to a law enforcement agency is underscored by examining ATF crime gun trace data for 2017<sup>15</sup> across the nine states that currently have some form of purchaser licensing. This data reveals a pattern consistent with previous research in state measures of within-state diversion to crime shortly after retail sale and the strength of the licensing law. We look at data for Illinois including Chicago and Illinois other than Chicago because Chicago has historically had very restrictive gun laws compared with the rest of the state. When Chicago data are excluded, Illinois, Iowa, and North Carolina – the only purchaser licensing states that don’t require fingerprints – have notably higher percentages of crime guns with time to crime under one year (15.9%, 21.4%, and 16.7%, respectively) and higher percentages of crime guns originating from within-state sales (65.0%, 71%, and 72.5%, respectively) than is the case for other purchaser licensing states that require in-person application, fingerprint verification and/or safety training including Maryland (11% under 12 months between retail sale and crime and 47.1% originating from within state).<sup>16</sup> By 2019, the share of Maryland’s crime guns with under 12 months to crime was 8% and 46% of the

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<sup>15</sup> Data obtained from the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) website for the year 2017 because Chicago Police Department released detailed crime gun trace data for that year. <https://www.atf.gov/resource-center/firearms-trace-data-2017>

<sup>16</sup> Hawaii has a relatively high percentage of its crime guns that were sold within a year of crime involvement; however, firearm ownership in Hawaii is very low and the number of crime guns traced (356) is a small fraction of the number of crime guns traced in other states.

state's crime guns originated from within state sales.<sup>17</sup> This is a clear signal that Maryland's HQL requirements that include fingerprinting applicants are effective in preventing firearms sold within the state from being diverted for criminal misuse.

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<sup>17</sup> <https://www.atf.gov/file/147101/download>

Percentage of crime guns recovered in 2017 within 12 months of sale and percentage of crime guns that were recovered in the source state among states with purchaser licensing handgun laws.

	Fingerprints Taken	Total guns recovered and successfully traced to purchaser	Crime guns recovered within 12 months of sale n (%)	Total guns recovered and successfully traced to first retail seller	Guns recovered that were purchased within the state n (%)
Connecticut	Yes	2,234	223 (10.0)	2,330	1,330 (57.1)
Hawaii	Yes	356	62 (17.4)	382	175 (45.8)
Illinois	No	30,749	4,428 (14.4)	31,336	15,752 (50.3)
Illinois minus Chicago		-18,526 Chicago 12,223	-2,483 Chicago 1,945 (15.9)	-18,749 Chicago 12,587	-7,575 Chicago 8,177 (65.0)
Iowa	No	4,462	953 (21.4)	4,521	3,221 (71.2)
Maryland	Yes	5,831	753 (12.9)	5,886	2,775 (47.1)
Massachusetts	Yes	4,243	498 (11.7)	4,342	1,696 (39.1)
New Jersey	Yes	9,263	747 (8.1)	9,355	2,027 (21.7)
New York	Yes	19,327	1,815 (9.4)	19,610	5,679 (29.0)
North Carolina	No	32,721	5,456 (16.7)	34,107	24,730 (72.5)

15. Kleck claims that it is simply not plausible that Missouri's PTP law or any handgun purchaser licensing law could impact firearm violence because relatively few criminals report that they obtained the firearms they used from licensed firearms dealers. There are two problems with that argument. First, the purchaser licensing requirement applies to private-party transfers as well as retail sales. Second, the point of background checks and licensing systems is not only to screen out prohibited persons, but to prevent the diversion of firearms after retail sale into the underground market through straw purchasers and gun traffickers. As I reported in my prior declarations, research that my colleagues and I published demonstrated that the adoption of Maryland's HQL requirement, along with other provisions of the Firearm Safety Act (FSA), was followed by an immediate 76% reduction in the number of handguns recovered in crime by Baltimore police that had originally been sold by a Maryland firearms dealer after controlling for the number of handguns sold (as reflected in background checks) and seasonal factors (Crifasi et al., 2017). In that study, we also conducted anonymous surveys of a sample of men on parole or probation in Baltimore. Among the respondents with history of involvement in the underground gun market, 40 percent said it became more difficult to get a gun after the 2013 FSA went into effect and 34 percent said it became more difficult to get someone to buy a gun for them after the law.

16. Kleck's declaration reveals that he either does not have a good understanding of firearm laws or does not consider the specifics of the laws that he discusses. For example, he claims that my research does not support the effects of fingerprint requirements for handgun purchase permits because McCourt et al. (2020) finds no effect of Maryland's law

that requires fingerprints. Kleck either fails to understand or misses the fact that the McCourt et al. (2020) study estimates the effects of Maryland's 1996 CBC law that extended background check requirement to private transfers but did not require fingerprints from purchase applicants. Of course, Maryland's requirements for fingerprints do not come into play until the Firearm Safety Act (FSA) went into effect in October 2013, the very law that is in question in this case. As we document in a separate study, Maryland's FSA requirement that handgun purchasers obtain a HQL was associated with an immediate and dramatic decrease in the diversion of firearms for criminal use after sales by Maryland gun dealers even after controlling for a reduction in the number of handguns sold following the law (Crifasi et al., 2017). Furthermore, 40 percent of persons on parole or probation for criminal offenses who were surveyed in Baltimore reported that it became harder to obtain firearms on the underground gun market following the 2013 law (Crifasi et al., 2017).

17. Kleck raises the question of why our team decided to focus on in-depth study on changes of handgun purchase laws in the four states of Maryland, Pennsylvania, Connecticut, and Missouri when the answer was obvious to those who read the article. We focus on four changes in laws because while they each apply only to handgun purchases, two concern comprehensive background check (CBC) requirements for all transfers (Maryland and Pennsylvania) and two concern changes in background check requirements in addition to purchaser permit/license requirements (Connecticut and Missouri). The two CBC laws were adopted within 12 months of the CBC and handgun purchaser license requirement in Connecticut, thus allowing us to estimate the relative importance of adding a license/permit requirement in addition to the CBC requirement. Of course, this is

precisely what Maryland did in 2013 when it added the HQL requirement to its CBC requirement that was enacted in 1996.

18. Kleck also incorrectly claims that we cherry picked states with permit-to-purchase handgun or license-to-own laws to study and points to eight states with purchase permit/license laws and three states with license-to-own laws, citing Giffords Law Center. Two of the eight purchase permit/license laws Kleck and Giffords identify, Nebraska and Michigan, do not meet our criteria for such laws because prospective purchasers are not required to have a permit or license to purchase a handgun from licensed firearm dealers in those states; the purchase permit requirement in those statutes only applies to private transfers.<sup>18</sup> This is a significant departure from a full purchaser licensing requirement. Nearly two-thirds of firearm acquisitions nationally are purchases from licensed firearms dealers (Miller, Hepburn, & Azrael, 2017).<sup>19</sup> All states that had a permit or license to purchase or own requirement for nearly all transfers, whether by a licensed dealer or an unlicensed (for retail sale) private party, were included in our study of the effects of state gun laws on homicides in urban counties. In this study, we estimate that these laws, on

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<sup>18</sup> As noted in on the same Giffords Law Center webpage cited by Kleck, “Michigan requires either a license to carry a concealed handgun or a handgun purchase license, although a person who purchases a handgun from a licensed dealer does not need either license... Nebraska issues handgun certificates, although handgun purchasers outside Omaha who purchase from licensed dealers or who have a concealed weapons permit do not need a handgun certificate.”

<https://giffords.org/lawcenter/gun-laws/policy-areas/owner-responsibilities/licensing/>

<sup>19</sup> Miller, Matthew, Lisa Hepburn, and Deborah Azrael. (2017) Firearm acquisitions without background checks: Results from a national survey. *Annals of Internal Medicine*. <https://www.acpjournals.org/doi/full/10.7326/M16-1590?journalCode=aim#t2-M161590>

average, reduced firearm homicide rates by 11 percent (Crifasi et al., 2018).<sup>20</sup> Thus, none of the relevant state laws have been left out of our collective body of work.

19. It is worth noting that some of the states with purchaser or owner licensing laws (Massachusetts, New York, New Jersey, North Carolina) date back 50 to 100 years. Our interest has been on understanding the impact of state gun laws in more recent years. Kleck criticizes our research because we do not use data dating back to the early 1930s, but he cites no published longitudinal studies that analyze changes in homicide trends over 80-90 years. I know of no such studies. There are good reasons that this is not done in research on the effects of public policies such as firearm laws. First, all statistical models designed to estimate the association between a policy change and any outcome is based on a projected counterfactual – what the model forecasts would have happened if there had been no policy change – and what actually occurred (the observed). Forecasting firearm homicide or suicide rates based on temporal patterns observed 30 to 90 years ago simply makes no sense and is likely to produce biased and inefficient (broader confidence intervals) estimates of policy effects. Forecasting counterfactuals over long periods after a policy intervention has been introduced also provides more opportunities for historical confounders to bias the estimates of policy impact. Whether you are forecasting the weather, the stock market, or homicide rates, there is greater uncertainty and increased

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<sup>20</sup> Crifasi, Cassandra K., Molly Merrill-Francis, Alexander D. McCourt, Jon S. Vernick, Garen J. Wintemute, and Daniel W. Webster. (2018) Association between Firearm Laws and Homicide in Large, Urban U.S. Counties, *Journal of Urban Health* 95(3):383-390. doi: 10.1007/s11524-018-0273-3. Correction: Oct 2018; 95 (5):773-776. [10.1007/s11524-018-0306-y](https://doi.org/10.1007/s11524-018-0306-y)

room for error the longer period you are attempting to forecast. Ironically, Kleck makes this same point in his rebuttal to our work when he claims that we are forecasting for too long of a period with our models. He seems to want to have it both ways. Some researchers who use synthetic control methods for such purposes, as we did in McCourt et al. (2020) and earlier studies of handgun purchaser licensing laws (Rudolph et al., 2015;<sup>21</sup> Crifasi et al., 2016<sup>22</sup>), and do not generate estimates beyond a 10-year period in which a policy has been in place due to concerns about the accuracy of forecasts over lengthy periods. Second, in addition to increasing the likelihood of bias in policy impact estimates, there is the issue of applicability to current times. While it would be interesting to know whether New York's PTP law reduced firearm homicides in the 1920s, I am doubtful that we could infer similar effects during the 21<sup>st</sup> century. There have been significant changes in society over those many decades that would influence rates of violent behavior, legal and illegal gun markets, and the guns themselves. Kleck portrays our decisions as if they are made to construct or inflate estimates to our liking when, in fact, these decisions are based on realistic understanding of what statistical models can and cannot do to generate valid estimates of the effects of laws. More historical data do not necessarily improve and can

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<sup>21</sup> Rudolph, Kara E., Elizabeth A. Stuart, Jon S. Vernick, and Daniel W. Webster. (2015) Association between Connecticut's permit-to-purchase handgun law and homicides. *American Journal of Public Health* 105(8):e49-54. doi:10.2105/AJPH.2015.302703.

<sup>22</sup> Crifasi, Cassandra K., John Speed Meyers, Jon S. Vernick JS, and Daniel W. Webster. (2015) Effects of changes in permit-to-purchase handgun laws in Connecticut and Missouri on suicide rates. *Preventive Med.* Jul 23, 2015. pii: S00917435(15)00229-7. doi: 10.1016/j.ypmed.2015.07.013. [Epub ahead of print] PMID: 26212633.



seriously bias estimates of policy effects when the time periods under study include many unmeasurable factors that influence the outcomes and primary independent variables under study (Donohue et al., 2019).

20. Kleck criticizes the study in which my colleagues and I show a strong negative association between handgun purchaser licensing laws and fatal mass shootings by suggesting that some unnamed confounder might explain the strong, statistically significant association – a 56 percent lower rate of the incidence of fatal mass shootings (Webster et al., 2020). There is no published research that suggests that any single factor could explain such a large difference in the rate of fatal mass shootings other than something that effectively restricts the availability of firearms, especially to persons at risk of committing violence. Kleck then puts forward something that he claims could explain such a strong association – failing to account for the effects of laws that prohibit persons who are dangerous due to mental illness from possessing firearms. However, if the estimated association between handgun purchaser licensing laws and a 56 percent lower rate of fatal mass shootings was due to our failure to control for laws that prohibit individuals from purchasing firearms due to mental illness, the following conditions would have to be met: (1) states and years with handgun purchaser licensing laws must have significantly broader mental health firearm prohibitions than states and years without handgun purchaser licensing; and (2) perpetrators of fatal mass shootings in states without handgun purchaser licensing laws must account for large majority of fatal mass shootings while close to no mass shooters in states with purchaser licensing laws would commit fatal mass shootings. Regarding the first point, Federal law – that obviously applies to all states

– prohibits persons from accessing firearms who have been found by a court, board, commission, or other lawful authority to be a danger to self or others, or to “lack[] the mental capacity to contract or manage [their] own affairs,” as a result of their mental condition or illness or who have been found incompetent to stand trial or not guilty of a crime due to mental incapacity.<sup>23</sup> Most state laws mimic the Federal prohibitions in their statutes (Giffords Law Center to Prevent Gun Violence, 2021).<sup>24</sup> A few states with handgun purchaser licensing laws that require in-person application with law enforcement or mandatory fingerprinting of applicants have somewhat broader firearm prohibitions based on an applicant’s mental illness than is the case under Federal law; yet those prohibitions still apply to a very small percentage of persons with a mental illness. During much of the study period (1984-2017), relatively few states submitted records for mental health disqualifiers into databases for background checks (Wintemute 2019). Only 5.3 percent of firearms transfer application denials in 2016 were for mental health related prohibitions

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<sup>23</sup> Federal law, enacted in 1968, still uses archaic and offensive terminology to prohibit firearm access by people who have been “adjudicated as a mental defective.” Federal regulations define that term to mean:

(a) A determination by a court, board, commission, or other lawful authority that a person, as a result of marked subnormal intelligence, or mental illness, incompetency, condition, or disease

(1) Is a danger to himself or to others; or

(2) Lacks the mental capacity to contract or manage his own affairs. Federal regulation also expressly clarifies that this firearm prohibition applies to:

(1) A finding of insanity by a court in a criminal case; and

(2) Those persons found incompetent to stand trial or found not guilty by reason of lack of mental responsibility pursuant to [specified articles] of the Uniform Code of Military Justice. 27 CFR § 478.11

<sup>24</sup> Giffords Law Center to Prevent Gun Violence. Who Can Have a Gun? Firearm Prohibitions. Accessed February 20, 2021. <https://giffords.org/lawcenter/gun-laws/policy-areas/who-can-have-a-gun/firearm-prohibitions/>

(Connor, 2021).<sup>25</sup> However, in 2007, only 0.8 percent of all denials for firearms transfers were due to mental health prohibitors (Federal Bureau of Investigation, 2007),<sup>26</sup> showing that the limited availability of mental health records during much of the study period greatly limited the ability of laws designed to keep firearms from people who are dangerous due to mental illness to impact gun violence.

21. Data from the most comprehensive dataset ([www.theviolenceproject.org](http://www.theviolenceproject.org)) on fatal mass violence that includes incidents beginning in 1966 includes data on 172 perpetrators of fatal mass shootings. The data indicate that 19.8 percent of the perpetrators had previously been hospitalized for psychiatric reasons and that psychosis played a primary role in 10.5 percent of the incidents (Peterson & Densley, 2021).<sup>27</sup> Typically, to be legally disqualified from acquiring firearms, someone must have been involuntarily committed for in-patient treatment for a mental illness or experienced a very lengthy (6+ months under Connecticut law) voluntary in-patient treatment that is very rare. Unfortunately, Peterson and Densley's (2021) data cannot reliably distinguish voluntary versus involuntary psychiatric hospitalizations. Involuntary civil commitments for psychiatric care are rare in comparison to voluntary psychiatric hospitalizations. In a study of 22,780 individuals identified with a serious mental illness in Connecticut based on

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<sup>25</sup> Brooks, Connor. (2021) Background Checks for Firearms Transfers, 2016-2017 – Statistical Tables. Bureau of Justice Statistics, Office for Justice Programs, U.S. Department of Justice. Washington, DC. NCJ 254757. <https://www.bjs.gov/content/pub/pdf/bcft15st.pdf>

<sup>26</sup> Federal Bureau of Investigations. National Instant Criminal Background System: Operations 2007. [https://www.fbi.gov/file-repository/2007\\_operations\\_report.pdf/view](https://www.fbi.gov/file-repository/2007_operations_report.pdf/view)

<sup>27</sup> Peterson, Jillian . (2021) Psychosis and Mass Shootings: A Systematic Examination using Publicly Available Data. Manuscript under review for publication.

administrative records from public mental health and criminal justice agencies, 1,086 (4.8 percent) had an involuntary civil commitment that prohibited firearm ownership and 1,122 (4.9 percent) had some kind of mental health prohibitor for possessing firearms and were not otherwise prohibited based on prior criminal convictions. Many perpetrators of mass shootings do, however, have histories of violence and criminality that would prohibit them from possessing firearms. In the Peterson and Densley (2021) comprehensive study of mass shootings in the United States, two-thirds (111 out of 172) had criminal records, usually for violent crimes. It is, therefore, far more likely that potential mass shooters are denied firearm purchases due to prior criminal offenses rather than for prohibiting conditions due to mental illness (Table 3). Given the very small number of potential mass shooters who might be denied due to mental illness and the fact that Federal law prohibits many if not most such persons from purchasing firearms, it is virtually impossible that the large association between handgun purchaser licensing laws and lower incidence of fatal mass shootings could be explained away by stronger mental health disqualifications for firearm possession in states with handgun purchaser licensing.

22. Kleck criticizes our article on state gun laws and fatal mass shootings because we did not demonstrate that the variables we controlled for in our models were, indeed, confounders for the relationship between purchaser licensing laws and fatal mass shootings. But most studies of gun laws do not formally test for these conditions. The best we can do as researchers is identify the most plausible covariates that are collected in a standardized way across all 50 states over decades of time. Fatal mass shootings are relatively rare events and there is not a large body of research showing which population

characteristics are most predictive of rates of fatal mass shootings. The study controls for the presence of 15 types of firearm laws and 13 other measures of potential population risk or conditions that may impact fatal mass violence including estimates of gun ownership, the most likely confounder.

23. Kleck also does not like that we did not tease apart the discrete impact of requiring fingerprints for handgun purchaser applicants versus the discrete impact of requiring applicants to apply for a handgun purchaser license in-person from a law enforcement agency. The reason we did not do this is that the study was designed to simultaneously assess the independent association between many different types of gun laws on a relatively rare outcome – fatal mass shooting with four or more deaths, not inclusive of the shooter. Only two states that had handgun purchaser licensing requirements during the study period had in-person application without fingerprint requirements (Iowa and Missouri) and only one state (Maryland) had fingerprint requirements without in-person application and that change in October 2013 provided very few annual data points for a study period that ended in 2017.

24. Kleck highlights the fact that the estimates for the association between handgun purchaser licensing laws and fatal mass shootings changed when we looked at the subset of cases in which more than five victims were killed versus our primary analyses with the commonly-used threshold of more than three victim fatalities. What he fails to mention is that the sample of incidents shrunk from 604 to 92 with those two different thresholds – 85 percent fewer incidents in which to analyze. As you restrict the fatality count threshold to higher numbers of deaths in these mass shootings, you find a growing

prevalence of assault-style rifles with high-capacity magazines which are not subject to handgun purchaser licensing. It is not surprising that the relationship between handgun purchaser licensing and fatal mass shootings would decline as the number killed threshold rises and assault rifles become more relevant.

25. Finally, Kleck claims that we biased our findings because we chose to exclude five states from our analysis for which there were many years of missing data from our primary source of data for the study – the FBI’s UCR Supplemental Homicide Report. We used our secondary sources – Stanford Mass Shooting in America and Gun Violence Archive – that did not cover all of the study period but allowed us to fill major mass shootings in when localities that were not reporting data (e.g., Newtown, Aurora, Colorado). Trying do to this for whole states for many years would have likely skewed our data.

26. Kleck makes an argument at the top of page 25 of his declaration regarding the findings from McCourt et al. (2020): “All of their (Mccourt et al.) results are completely consistent with the interpretation that these changes, if they had any actual impact at all, merely induced some people to change the weapons they used to kill others, or the methods they used to kill themselves, without any effect on the total number who died.” The argument he puts forward is that firearm restrictions will just lead to method substitution such that changes in firearm homicide and suicide rates associated with changes in purchaser licensing laws would be negated by opposing changes in non-firearm homicide and suicide rates. A close inspection of the data in Table 2 on page 1549 of the article, puts that claim to rest. While we found that Connecticut’s handgun purchaser

licensing law was associated with a 27.8 percent decline in firearm homicide rates, it was associated with a non-significant 0.7 percent decline in non-firearm homicide rates – in essence, no change. The estimated change in firearm suicide rates was 23 percent during the period prior to Connecticut’s firearm removal law (precursor to Red Flag laws) and a combined reduction of 32 percent for the whole study period. Under Kleck’s hypothesis, some significant amount of that reduction would be negated by an increase in non-firearm suicides. Yet our models estimate a non-significant 3 percent decline in non-firearm suicide rates associate with Connecticut’s purchaser licensing law. The same is true when we examine the data for Missouri. We estimate that the purchaser licensing law repeal was associated with a 47 percent increase in firearm homicide rates. These increases in firearm homicides were not negated by decreases in non-firearm homicides, which also increased but at a much smaller rate of 18 percent. The pattern for suicide rates changing in response to changes in handgun purchaser licensing laws follow the same pattern as with homicides. The 23.5 percent increase in firearm suicide rates following the repeal of Missouri’s handgun purchaser law was not accompanied by a decline in non-firearm suicides, but a nonsignificant increase of 6.9 percent. We present the data stratified by weapon type to show that the changes observed in response to changes in handgun purchaser licensing laws are specific to or primarily concentrated on changes in firearm homicide and suicide and have little or no impact on homicides and suicides by other means. Not a single analysis in McCourt et al. (2020) supported Kleck’s method substitution hypothesis. The analyses simply reveal that the observed changes in homicide and suicide rates in response to

changes in purchaser licensing laws were highly concentrated in, if not exclusive to, changes in deaths due to firearms.

27. Kleck says that the authors' study periods in McCourt et al. (2020) are arbitrary and provides our analysis of Maryland's CBC law as an example where our analysis ends at 2013 – 17 years of post-law data. As we state in our article, we truncate Maryland's time series at 2013 because our purpose is to contrast CBC versus handgun purchaser licensing laws and Maryland's begins requiring handgun purchaser licensing in October 2013. Adding four years of post-CBC data that included these new important law change would only bias our estimate of the CBC effects. Finally, Baltimore City's firearm homicide rates skyrocketed in May 2015 and have remained at high levels after massive destructive riots in response to the in-custody death of Freddie Gray. Kleck disagrees with our decision to truncate our post-CBC time series for Maryland at 2013 and our post-PTP-repeal time series for Missouri to 2017 due to the adoption of significant changes firearm laws in those states that prior research has shown to impact firearm homicide rates. His argument is that gun laws change all of the time, so researchers are not justified in truncating data for any reason. This is foolhardy because many of the changes in gun laws are very modest in nature and unlikely to impact population-level homicide rates whereas other law changes are more substantial and have been shown to be associated with changes in firearm homicide rates. The purpose and study design for McCourt et al. (2020) was to isolate changes in key policies by analyzing trends over sufficiently long periods in which there are no other major policy changes. For Maryland's CBC law, we used 17 years of post-law change data and for Missouri we used 10 years of post-law data. Forecasting



counterfactuals beyond those time periods with significant policy changes coming into effect would not improve and would most likely bias our estimates of CBC and PTP law change effects.

28. Kleck says of our analytic method for estimating the effects of policy changes on homicide and suicide rates – synthetic control methods – that they “might be useful for evaluating the impact of a policy, but only in extraordinary circumstances.” Yet synthetic control methodology has become a widely accepted, and often preferred, method for estimating policy impacts. Synthetic control methodology has been called arguably the most important innovation in the policy evaluation literature in the last 15 years by two of the most prominent economists in the world who are studying policy impacts (Athey and Imbens, 2017),<sup>28</sup> and the key articles on synthetic control methodology have over 4,000 citations. Other methods of estimating policy impacts, such as difference-in-difference estimates<sup>29</sup> from regression analysis with panel data are premised on assumptions of parallel trends in treated and untreated units prior to treatment (policy change in this case) and assumptions about the proper form of the relationship between the independent and dependent variables in the analysis. These conditions often do not hold in studies of state-level changes in homicides and suicides. This is why my colleagues and I chose not to use data from the 1980s and 1990s in our studies of the impacts of Missouri’s repeal of its

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<sup>28</sup> Athey, Susan. and Guido W. Imbens (2017). The state of applied econometrics: Causality and policy evaluation. *Journal of Economic Perspectives* 31 (2), 3–32.

<sup>29</sup> Differences-in-differences refers to differences in dependent variables before and after policy change in the jurisdiction with the change versus the difference in the dependent variable over the same two time periods among jurisdictions that did not change the policy after controlling for changes in variables controlled for in the statistical model.

handgun purchaser licensing law when we used negative binomial regression models (Webster et al., 2014; Hasagawa, Webster & Smart, 2019). Synthetic control models minimize prediction error by allowing for different comparison states to receive different weights depending upon their ability to aid in the prediction of homicide or suicide trends in the state with the policy change under study. It is a mathematical process that is the opposite of “cherry picking” because it selects weights based on the weights’ ability to minimize prediction error. Kleck is incorrect in his statement that synthetic control methodology “relies entirely on the coincidence of there being other areas whose trends in the outcome variable closely mirror those prevailing in the area in which the policy is implemented.” Obviously, the more similar the comparison units are with respect to baseline levels and trends, the easier it is for the method to derive a good fit or prediction with the model. The estimation methodology does not rely upon parallel trends of comparison units and this is precisely the reason so many researchers use this method (Bouttell et al., 2017). My colleagues and I recently published a study that demonstrated substantial improvement in model fit when synthetic control methods were used in comparison to difference-in-difference estimates from regression analysis in a study of local gun violence prevention interventions in Baltimore neighborhoods – another type of study in which study units can be quite different and have unique, nonparallel trends.<sup>30</sup>

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<sup>30</sup> Buggs, Shani A.L., Daniel W. Webster, and Cassandra K. Crifasi. (2021) Using synthetic control methodology to estimate effects of a Cure Violence intervention in Baltimore, Maryland. *Injury Prevention* Published Online First: 08 February 2021. <http://dx.doi.org/10.1136/injuryprev-2020-044056>.

29. Kleck claims that the McCourt et al. (2020) study does not provide any evidence that the variables we used in our statistical models were essential in predicting homicide or suicide trends. Yet we provide model fit statistics and graphs of actual versus model-predicted values that show remarkable strong model fit and thus strong prediction based on the variables we used. Kleck picks apart data point by data point yearly change in Missouri or Connecticut prior to the law change to support his claim that the synthetic control method we used did not work well. But the approach is designed to estimate the value of the dependent variable for each year over a span of time, minimizing error in the mean values rather than minimizing error predicting a 1-year change. The graphs shows close concordance with the mean and general trend prior to the law's repeal in Missouri and Connecticut, especially close concordance during the years leading up to the policy change – key in any estimation of policy effects. The model fit statistics – mean square predictive error – for Missouri's firearm homicide rates and all other models were all less than 0.560.

30. Finally, Kleck argues that we cannot conclude that firearm homicide and suicide rates changed dramatically in response to changes in handgun purchaser licensing laws in Missouri and Connecticut because it is possible that it was some other factor or set of factors that changed at the same time the laws were changing that dramatically influenced firearm homicide and suicide rates. While this is theoretically possible in any study of a law because we cannot run randomized experiments with laws, the pattern and consistency of the data are in accord with my inference that handgun purchaser licensing lowers population rates of firearm homicide and suicide. The effects are substantial and

long-lasting, are evident primarily with firearm homicides and suicides and not with homicides and suicides by other means. The changes in diversions of firearms for criminal use that coincide with the changes in firearm homicide rates are consistent with a causal connection.

I hereby declare under penalty of perjury that the foregoing is true and correct.

Date: February 24, 2021



Daniel W. Webster